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THE ZOOLOGIST:

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POPULAR MONTHLY MAGAZINE

OF

NATURAL HISTORY

AND

JOURNAL FOR RECORDING FACTS & ANECDOTES

RELATING TO

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LATE PRESIDENT OF THE ENTOMOLOGICAL SOCIETY.

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II. Abstract of a Letter from C. DARWIN, Esq., to Prof. ASA GRAY, Boston, U.S., dated Down, September 5th, 1857.

1. It is wonderful what the principle of selection by man, that is the picking out of individuals with any desired quality, and breeding from them, and again picking out, can do. Even breeders have been astounded at their own results. They can act on differences inappreciable to an uneducated eye. Selection has been *methodically* followed in *Europe* for only the last half-century; but it was occasionally, and even in some degree methodically, followed in the most ancient times. There must have been also a kind of unconscious selection from a remote period, namely, in the preservation of the individual animals (without any thought of their offspring) most useful to each race of man in his particular circumstances. The "roguing," as nurserymen call the destroying of varieties which depart from their type, is a kind of selection. I am convinced that intentional and occasional selection has been the main agent in the production of our domestic races; but however this may be, its great power of modification has been indisputably shown in later times. Selection acts only by the accumulation of slight or greater variations, caused by external conditions, or by the mere fact that in generation the child is not absolutely similar to its parent. Man, by this power of accumulating variations, adapts living beings to his wants,—may be said to make the wool of one sheep good for carpets, of another for cloth, &c.

2. Now suppose there were a being who did not judge by mere external appearances, but who could study the whole internal organization, who was never capricious, and should go on selecting for one object during millions of generations; who will say what he might not effect? In nature we have some *slight* variation occasionally in all parts; and I think it can be shown that changed conditions of existence is the main cause of the child not exactly resembling its parents; and in nature Geology shows us what changes have taken place, and are taking place. We have almost unlimited time; no one but a practical geologist can fully appreciate this. Think of the Glacial period, during the whole of which the same species at least of shells have existed; there must have been during this period millions on millions of generations.

3. I think it can be shown that there is such an unerring power at work in *Natural Selection* (the title of my book), which selects exclu-

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sively for the good of each organic being. The elder DeCandolle, W. Herbert, and Lyell have written excellently on the struggle for life; but even they have not written strongly enough. Reflect that every being (even the elephant) breeds at such a rate that in a few years, or at most a few centuries, the surface of the earth would not hold the progeny of one pair. I have found it hard constantly to bear in mind that the increase of every single species is checked during some part of its life, or during some shortly recurrent generation. Only a few of those annually born can live to propagate their kind. What a trifling difference must often determine which shall survive, and which perish!

4. Now take the case of a country undergoing some change. This will tend to cause some of its inhabitants to vary slightly — not but that I believe most beings vary at all times enough for selection to act on them. Some of its inhabitants will be exterminated, and the remainder will be exposed to the mutual action of a different set of inhabitants, which I believe to be far more important to the life of each being than mere climate. Considering the infinitely various methods which living beings follow to obtain food by struggling with other organisms, to escape danger at various times of life, to have their eggs or seeds disseminated, &c. &c., I cannot doubt that during millions of generations individuals of a species will be occasionally born with some slight variation, profitable to some part of their economy. Such individuals will have a better chance of surviving, and of propagating their new and slightly different structure; and the modification may be slowly increased by the accumulative action of natural selection to any profitable extent. The variety thus formed will either coexist with, or, more commonly, will exterminate its parent form. An organic being, like the woodpecker or misseltoe, may thus come to be adapted to a score of contingencies—natural selection accumulating those slight variations in all parts of its structure, which are in any way useful to it during any part of its life.

5. Multiform difficulties will occur to every one, with respect to this theory. Many can, I think, be satisfactorily answered. *Natura non facit saltum* answers some of the most obvious. The slowness of the change, and only a very few individuals undergoing change at any one time, answers others. The extreme imperfection of our geological records answers others.

6. Another principle, which may be called the principle of divergence, plays, I believe, an important part in the origin of species. The same spot will support more life if occupied by very diverse forms.

We see this in the many generic forms in a square yard of turf, and in the plants or insects on any little uniform islet, belonging almost invariably to as many genera and families as species. We can understand the meaning of this fact amongst the higher animals, whose habits we understand. We know that it has been experimentally shown that a plot of land will yield a greater weight if sown with several species and genera of grasses, than if sown with only two or three species. Now, every organic being, by propagating so rapidly, may be said to be striving its utmost to increase in numbers. So it will be with the offspring of any species after it has become diversified into varieties, or subspecies, or true species. And it follows, I think, from the foregoing facts, that the varying offspring of each species will try (only a few will succeed) to seize on as many and as diverse places in the economy of nature as possible. Each new variety or species, when formed, will generally take the place of, and thus exterminate its less well-fitted parent. This I believe to be the origin of the classification and affinities of organic beings at all times; for organic beings always *seem* to branch and sub-branch like the limbs of a tree from a common trunk, the flourishing and diverging twigs destroying the less vigorous —the dead and lost branches rudely representing extinct genera and families.

This sketch is *most* imperfect; but in so short a space I cannot make it better. Your imagination must fill up very wide blanks.

C. DARWIN.

III. On the Tendency of Varieties to depart indefinitely from the Original Type.

By ALFRED RUSSEL WALLACE.

One of the strongest arguments which have been adduced to prove the original and permanent distinctness of species is, that *varieties* produced in a state of domesticity are more or less unstable, and often have a tendency, if left to themselves, to return to the normal form of the parent species; and this instability is considered to be a distinctive peculiarity of all varieties, even of those occurring among wild animals in a state of nature, and to constitute a provision for preserving unchanged the originally created distinct species.

In the absence or scarcity of facts and observations as to *varieties* occurring among wild animals, this argument has had great weight with naturalists, and has led to a very general and somewhat prejudiced belief in the stability of species. Equally general, however, is the belief in what are called "permanent or true varieties,"--races of animals which continually propagate their like, but which differ so slightly (although constantly) from some other race, that the one is considered to be a *variety* of the other. Which is the *variety* and which the original *species*, there is generally no means of determining, except in those rare cases in which the one race has been known to produce an offspring unlike itself and resembling the other. This, however, would seem quite incompatible with the "permanent invariability of species," but the difficulty is overcome by assuming that such varieties have strict limits, and can never again vary further from the original type, although they may return to it, which, from the analogy of the domesticated animals, is considered to be highly probable, if not certainly proved.

It will be observed that this argument rests entirely on the assumption, that *varieties* occurring in a state of nature are in all respects analogous to or even identical with those of domestic animals, and are governed by the same laws as regards their permanence or further variation. But it is the object of the present paper to show that this assumption is altogether false, that there is a general principle in nature which will cause many *varieties* to survive the parent species, and to give rise to successive variations departing further and further from the original type, and which also produces, in domesticated animals, the tendency of varieties to return to the parent form.

The life of wild animals is a struggle for existence. The full exertion of all their faculties and all their energies is required to preserve their own existence and provide for that of their infant offspring. The possibility of procuring food during the least favourable seasons, and of escaping the attacks of their most dangerous enemies, are the primary conditions which determine the existence both of individuals and of entire species. These conditions will also determine the population of a species; and by a careful consideration of all the circumstances we may be enabled to comprehend, and in some degree to explain, what at first sight appears so inexplicable—the excessive abundance of some species, while others closely allied to them are very rare.

The general proportion that must obtain between certain groups of animals is readily seen. Large animals cannot be so abundant as small ones; the Carnivora must be less numerous than the Herbivora; eagles and lions can never be so plentiful as pigeons and antelopes; the wild asses of the Tartarian deserts cannot equal in numbers the horses of the more luxuriant prairies and pampas of America. The greater or less fecundity of an animal is often considered to be one of the chief causes of its abundance or scarcity; but a consideration of the facts will show us that it really has little or nothing to do with the matter. Even the least prolific of animals would increase rapidly if unchecked, whereas it is evident that the animal population of the globe must be stationary, or perhaps, through the influence of man, decreasing. Fluctuations there may be; but permanent increase, except in restricted localities, is almost impossible. For example, our own observation must convince us that birds do not go on increasing every year in a geometrical ratio, as they would do were there not some powerful check to their natural increase. Very few birds produce less than two young ones each year, while many have six, eight or ten; four will certainly be below the average; and if we suppose that each pair produce young only four times in their life, that will also be below the average, supposing them not to die either by violence or want of food. Yet at this rate how tremendous would be the increase in a few years from a single pair! A simple calculation will show that in fifteen years each pair of birds would have increased to nearly ten millions! whereas we have no reason to believe that the number of the birds of any country increases at all in fifteen or in one hundred and fifty years. With such powers of increase the population must have reached its limits, and have become stationary, in a very few years after the origin of each species. It is evident, therefore, that each year an immense number of birds must perish—as many in fact as are born; and as on the lowest calculation the progeny are each year twice as numerous as their parents, it follows that, whatever be the average number of individuals existing in any given country, *twice that number must perish annually*,—a striking result, but one which seems at least highly probable, and is perhaps under rather than over the truth. It would therefore appear that, as far as the continuance of the species and the keeping up the average number of individuals are concerned, large broods are superfluous. On the average all above *one* become food for hawks and kites, wild cats and weasels, or perish of cold and hunger as winter comes on. This is strikingly proved by the case of particular species; for we find that their abundance in individuals bears no relation whatever to their fertility in producing offspring. Perhaps the most remarkable instance of an immense bird population is that of the passenger pigeon of the United States, which lays only one, or at most two eggs, and is said to rear generally but one young one. Why is this bird so extraordinarily abundant, while others producing two or three times as many young are much less plentiful? The explanation is not difficult. The food most congenial to this species, and on which it thrives best, is abundantly distributed over a very extensive region, offering such differences of soil and climate, that in one part or another of the area the supply never fails. The bird is capable of a very rapid and long-continued flight, so that it can pass without fatigue over the whole of the district it inhabits, and as soon as the supply of food begins to fail in one place is able to discover a fresh feeding-ground. This example strikingly shows us that the procuring a constant supply of wholesome food is almost the sole condition requisite for ensuring the rapid increase of a given species, since neither the limited fecundity, nor the unrestrained attacks of birds of prey and of man are here sufficient to check it. In no other birds are these peculiar circumstances so strikingly combined. Either their food is more liable to failure, or they have not sufficient power of wing to search for it over an extensive area, or during some season of the year it becomes very scarce, and less wholesome substitutes have to be found; and thus, though more fertile in offspring, they can never increase beyond the supply of food in the least favourable seasons. Many birds can only exist by migrating, when their food becomes scarce, to regions possessing a milder, or at least a different climate, though, as these migrating birds are seldom excessively abundant, it is evident that the countries they visit are still deficient in a constant and abundant supply of wholesome food. Those whose organization does not permit them to migrate when their food becomes periodically scarce, can never attain a large population. This is probably the reason why woodpeckers are scarce with us, while in the tropics they are among the most abundant of solitary birds. Thus the house sparrow is more abundant than the redbreast, because its food is more constant and plentiful,—seeds of grasses being preserved during the winter, and our farm-yards and stubble-fields furnishing an almost inexhaustible supply. Why, as a general rule, are aquatic, and especially sea birds, very numerous in individuals? Not because they are more prolific than others, generally the contrary; but because their food never fails, the sea-shores and river-banks daily swarming with a fresh supply of small Mollusca and Crustacea. Exactly the same laws will apply to mammals. Wild cats are prolific and have few enemies; why then are they never as abundant as rabbits? The only intelligible answer is, that their supply of food is more precarious. It appears evident, therefore, that so long as a country remains physically unchanged, the numbers of its animal population cannot materially increase. If one species does so, some others requiring the same kind of food must diminish in proportion. The numbers that die annually must be immense; and as the individual existence of each animal depends upon itself, those that die must be the weakest—the very young, the aged, and the diseased,—while those that prolong their existence can only be the most perfect in health and vigour—those who are best able to obtain food regularly, and avoid their numerous enemies. It is, as we commenced by remarking, "a struggle for existence," in which the weakest and least perfectly organized must always succumb.

Now it is clear that what takes place among the individuals of a species must also occur among the several allied species of a group,—*viz.*, that those which are best adapted to obtain a regular supply of food, and to defend themselves against the attacks of their enemies and the vicissitudes of the seasons, must necessarily obtain and preserve a superiority in population; while those species which from some defect of power or organization are the least capable of counteracting the vicissitudes of food, supply, &c., must diminish in numbers, and, in extreme cases, become altogether extinct. Between these extremes the species will present various degrees of capacity for ensuring the means of preserving life; and it is thus we account for the abundance or rarity of species. Our ignorance will generally prevent us from accurately tracing the effects to their causes; but could we become perfectly acquainted with the organization and habits of the various species of animals, and could we measure the capacity of each for performing the different acts necessary to its safety and existence under all the varying circumstances by which it is surrounded, we might be able even to calculate the proportionate abundance of individuals which is the necessary result.

If now we have succeeded in establishing these two points—1st, *that the animal population of a country is generally stationary, being kept down by a periodical deficiency of food, and other checks;* and, 2nd, *that the comparative abundance or scarcity of the individuals of the several species is entirely due to their organization and resulting habits, which, rendering it more difficult to procure a regular supply of food and to provide for their personal safety in some cases than in others, can only be balanced by a difference in the population which have to exist in a given area*—we shall be in a condition to proceed to the consideration of *varieties*, to which the preceding remarks have a direct and very important application.

Most or perhaps all the variations from the typical form of a species must have some definite effect, however slight, on the habits or capacities of the individuals. Even a change of colour might, by rendering them more or less distinguishable, affect their safety; a greater or less development of hair might modify their habits. More important changes, such as an increase in the power or dimensions of the limbs or any of the external organs, would more or less affect their mode of procuring food or the range of country which they inhabit. It is also evident that most changes would affect, either favourably or adversely, the powers of prolonging existence. An antelope with shorter or weaker legs must necessarily suffer more from the attacks of the feline Carnivora; the passenger pigeon with less powerful wings would sooner or later be affected in its powers of procuring a regular supply of food; and in both cases the result must necessarily be a diminution of the population of the modified species. If, on the other hand, any species should produce a variety having slightly increased powers of preserving existence, that variety must inevitably in time acquire a superiority in numbers. These results must follow as surely as old age, intemperance, or scarcity of food produce an increased mortality. In both cases there may be many individual exceptions; but on the average the rule will invariably be found to hold good. All varieties will therefore fall into two classes—those which under the same conditions would never reach the population of the parent species, and those which would in time obtain and keep a numerical superiority. Now, let some alteration of physical conditions occur in the district—a long period of drought, a destruction of vegetation by locusts, the irruption of some new carnivorous animal seeking "pastures new"—any change in fact tending to render existence more difficult to the species in question, and tasking its utmost powers to avoid complete extermination; it is evident that, of all the individuals composing the species, those forming the least numerous and most feebly organized variety would suffer first, and, were the pressure severe, must soon become extinct. The same causes continuing in action, the parent species would next suffer, would gradually diminish in numbers, and with a recurrence of similar unfavourable conditions might also become extinct. The superior variety would then alone remain, and on a return to favourable circumstances would rapidly increase in numbers and occupy the place of the extinct species and variety.

The *variety* would now have replaced the *species*, of which it would be a more perfectly developed and more highly organised form. It would be in all respects better adapted to secure its safety, and to prolong its individual existence and that of the race. Such a variety *could* *not* return to the original form; for that form is an inferior one, and could never compete with it for existence. Granted, therefore a "tendency" to reproduce the original type of the species, still the variety must ever remain preponderant in numbers, and under adverse physical conditions *again alone survive.* But this new, improved, and populous race might itself, in course of time, give rise to new varieties, exhibiting several diverging modifications of form, any of which, tending to increase the facilities for preserving existence, must, by the same general law, in their turn become predominant. Here, then, we have *progression and continued divergence* deduced from the general laws which regulate the existence of animals in a state of nature, and from the undisputed fact that varieties do frequently occur. It is not, however, contended that this result would be invariable; a change of physical conditions in the district might at times materially modify it, rendering the race which had been the most capable of supporting existence under the former conditions now the least so, and even causing the extinction of the newer, and for a time superior, race, while the old or parent species and its first inferior varieties continued to flourish. Variations in unimportant parts might also occur, having no perceptible effect on the life-preserving powers; and the varieties so furnished might run a course parallel with the parent species, either giving rise to further variations or returning to the former type. All we argue for is, that certain varieties have a tendency to maintain their existence longer than the original species, and this tendency must make itself felt; for though the doctrine of chances or averages can never be trusted to on a limited scale, yet, if applied to high numbers, the results come nearer to what theory demands, and, as we approach to an infinity of examples, become strictly accurate. Now the scale on which nature works is so vast—the numbers of individuals and periods of time with which she deals approach so near to infinity, that any cause, however slight, and however liable to be veiled and counteracted by accidental circumstances, must in the end produce its full legitimate results.

Let us now turn to domesticated animals, and inquire how varieties produced among them are affected by the principles here enunciated. The essential difference in the condition of wild and domestic animals is this,—that among the former, their well-being and very existence depend upon the full exercise and healthy condition of all their senses and physical powers, whereas, among the latter, these are only partially exercised, and in some cases are absolutely unused. A wild animal has to search, and often to labour, for every mouthful of food

—to exercise sight, hearing, and smell in seeking it, and in avoiding dangers, in procuring shelter from the inclemency of the seasons, and in providing for the subsistence and safety of its offspring. There is no muscle of its body that is not called into daily and hourly activity; there is no sense or faculty that is not strengthened by continual exercise. The domestic animal, on the other hand, has food provided for it, is sheltered, and often confined, to guard it against the vicissitudes of the seasons, is carefully secured from the attacks of its natural enemies, and seldom even rears its young without human assistance. Half of its senses and faculties are quite useless; and the other half are but occasionally called into feeble exercise, while even its muscular system is only irregularly called into action.

Now when a variety of such an animal occurs, having increased power or capacity in any organ or sense, such increase is totally useless, is never called into action, and may even exist without the animal ever becoming aware of it. In the wild animal, on the contrary, all its faculties and powers being brought into full action for the necessities of existence, any increase becomes immediately available, is strengthened by exercise, and must even slightly modify the food, the habits, and the whole economy of the race. It creates as it were a new animal, one of superior powers, and which will necessarily increase in numbers and outlive those inferior to it.

Again, in the domesticated animal all variations have an equal chance of continuance; and those which would decidedly render a wild animal unable to compete with its fellows and continue its existence are no disadvantage whatever in a state of domesticity. Our quickly fattening pigs, short-legged sheep, pouter pigeons and poodle dogs could never have come into existence in a state of nature, because the very first step towards such inferior forms would have led to the rapid extinction of the race; still less could they now exist in competition with their wild allies. The great speed but slight endurance of the race horse, the unwieldy strength of the ploughman's team, would both be useless in a state of nature. If turned wild on the pampas, such animals would probably soon become extinct, or under favourable circumstances might each lose those extreme qualities which would never be called into action, and in a few generations would revert to a common type, which must be that in which the various powers and faculties are so proportioned to each other as to be best adapted to procure food and secure safety,—that in which by the full exercise of every part of his organization the animal can alone continue to live. Domestic varieties, when turned wild, *must* return to something near the type of the original wild stock, *or become altogether extinct.*

We see, then, that no inferences as to varieties in a state of nature can be deduced from the observation of those occurring among domestic animals. The two are so much opposed to each other in every circumstance of their existence, that what applies to the one is almost sure not to apply to the other. Domestic animals are abnormal, irregular, artificial; they are subject to varieties which never occur and never can occur in a state of nature: their very existence depends altogether on human care; so far are many of them removed from that just proportion of faculties, that true balance of organization, by means of which alone an animal left to its own resources can preserve its existence and continue its race.

The hypothesis of Lamarck—that progressive changes in species have been produced by the attempts of animals to increase the development of their own organs, and thus modify their structure and habits —has been repeatedly and easily refuted by all writers on the subject of varieties and species, and it seems to have been considered that when this was done the whole question has been finally settled; but the view here developed renders such an hypothesis quite unnecessary, by showing that similar results must be produced by the action of principles constantly at work in nature. The powerful retractile talons of the falcon and the cat tribes have not been produced or increased by the volition of those animals; but among the different varieties which occurred in the earlier and less highly organized forms of these groups, *those always survived longest which had the greatest facilities for seizing their prey.* Neither did the giraffe acquire its long neck by desiring to reach the foliage of the more lofty shrubs, and constantly stretching its neck for the purpose, but because any varieties which occurred among its anti-types with a longer neck than usual *at once secured a fresh range of pasture over the same ground as their shorter-necked companions, and on the first scarcity of food were thereby enabled to outlive them.* Even the peculiar colours of many animals, especially insects, so closely resembling the soil or the leaves or the trunks on which they habitually reside, are explained on the same principle; for though in the course of ages varieties of many tints may have occurred, *yet those races having colours best adapted to concealment from their enemies would inevitably survive the longest.* We have also here an acting cause to account for that balance so often observed in nature,—a deficiency in one set of organs always being compensated by an increased development of some others—powerful wings accompanying weak feet, or great velocity making up for the absence of defensive weapons; for it has been shown that all varieties in which an unbalanced deficiency occurred could not long continue their existence. The action of this principle is exactly like that of the centrifugal governor of the steam engine, which checks and corrects any irregularities almost before they become evident; and in like manner no unbalanced deficiency in the animal kingdom can ever reach any conspicuous magnitude, because it would make itself felt at the very first step, by rendering existence difficult and extinction almost sure soon to follow. An origin such as is here advocated will also agree with the peculiar character of the modifications of form and structure which obtain in organized beings—the many lines of divergence from a central type, the increasing efficiency and power of a particular organ through a succession of allied species, and the remarkable persistence of unimportant parts, such as colour, texture of plumage and hair, form of horns or crests, through a series of species differing considerably in more essential characters. It also furnishes us with a reason for that "more specialized structure" which Professor Owen states to be a characteristic of recent compared with extinct forms, and which would evidently be the result of the progressive modification of any organ applied to a special purpose in the animal economy.

We believe we have now shown that there is a tendency in nature to the continued progression of certain classes of *varieties* further and further from the original type—a progression to which there appears no reason to assign any definite limits—and that the same principle which produces this result in a state of nature will also explain why domestic varieties have a tendency to revert to the original type. This progression, by minute steps, in various directions, but always checked and balanced by the necessary conditions, subject to which alone existence can be preserved, may, it is believed, be followed out so as to agree with all the phenomena presented by organized beings, their extinction and succession in past ages, and all the extraordinary modifications of form, instinct, and habits which they exhibit.

A. R. WALLACE.

Ternate, February, 1858.

Weasel carrying its Young.—On the 18th of September I saw a weasel run across the road, with a young one in its mouth, apparently about a month old. I write these particulars as I think it is not generally known that weasels do so carry their young ones.—*Howard Fox; Falmouth, October* 13, 1858.

Occurrence of Crossbills in Yorkshire.—Last week nine crossbills (*L. curvirostra*) were killed, out of a flock of eleven, about nine miles above Richmond. They had constantly frequented the fir plantation in which they were killed, since last February, and had they remained undisturbed I think it quite possible that they would have bred there this winter.—*H. Smurthwaite; Bank, Richmond, Yorkshire, November* 2, 1858.

Remarkable Anecdote of the Cuckoo.—At the village of Tickenhall, in Derbyshire, lime is procured for agricultural purposes, which, of course, every time it is blasted makes a great noise. Notwithstanding all the noise and bustle attendant upon the operations of procuring it, a small bird built its nest close by a bush which grew upon a rough bank. The egg of a cuckoo was laid in it in due time, and eventually hatched. The young cuckoo did not seem at all frightened by what was going on around it. When fully fledged, a friend of mine took it, and gave it to his little boy to make a pet of. It lived upon eggs, caterpillars, flies, insects of various kinds, and raw beef, and throve remarkably well. It was pinioned, and allowed to roam about the orchards and neighbouring fields, and grew tame. One day, however, it was missing, and was away a whole month. It was found, however, within a few fields of the house, and, singular to say, several little wild birds were in the act of feeding it. The operation was watched for some time with much interest. It seems very remarkable that having been in partial confinement for perhaps three weeks or a month, the birds should feed it. It would almost lead one to think that in the cuckoo existed something peculiar and attractive which induced other birds to take it under their protection.—*John Joseph Briggs in 'The Field.'*

The Creamcoloured Courser in Hackney Marshes.—I am indebted to Mr. Cooper, of No. 28, Radnor Street, St. Luke's, for sending me, in the flesh, a very perfect female specimen of that rare bird the Cursorius isabellinus of naturalists. It was shot in the Hackney Marshes, on the 19th of October, by Mr. George Beresford, of the White House Fishery. The bird was exceedingly tame, probably tired with a long flight. The last recorded instance of the occurrence of this bird in Britain is in the 'Zoologist' for 1855 (Zool. 4913). The specimen was shot by Mr. Walter Langton, on East Down, Salisbury Plain, on the 2nd of October, 1855. Mr. Langton was following a wild covey of partridges which had pitched on the open down, when his dogs stood at this bird; it rose, flew about a hundred yards, and settled again. He followed it, and finally shot it on the ground. The late Mr. Yarrell saw and handled the bird in the flesh; I had the same pleasure. Mr. Cooper will be pleased to show his more recent specimen to any gentleman who inclines to call on him at the above address.—*Edward Newman in 'The Field.'*

Appearance of Skuas off the Coast of Yarmouth.—During the past month, several fine specimens of the common skua, mostly in adult plumage, have been killed off Yarmouth, as well as six specimens of Richardson's and one pomarine skua. Of Richardson's skuas three were in the immature plumage of the second year, an uniform brownish tint, with the middle tail feathers projecting about three inches, and the others had attained very nearly their adult state, the under parts being white with the exception of a few dusky markings remaining more or less on the throat and breast: in this plumage they are very rarely met with on this coast. The single pomarine specimen that I examined was also in rapid progress to maturity, the whole of the under surface of the body being mottled with white and dusky brown. All these birds were shot from the boats engaged in the herring fishery, as the skuas, like the common gulls, follow up the shoals of fish as they approach our shores, but not with the same honest intention of gaining their own living as their kindred species, whom they too often bully out of their well-earned meal, as the sea eagle pounces on the osprey.—*H. Stevenson; Norwich, November* 6, 1858,

Colias Edusa near London.—On Wednesday, October 27th, I took a fine male specimen of Colias Edusa in my garden, apparently fresh from the chrysalis. I have recorded this fact, thinking it somewhat remarkable in this locality, so near London. Since writing the above my son has taken another specimen of C. Edusa in the Camden Road, leading to the villas.—*Joshua Dix; 29, St. Paul's Road, Camden Square, N.W., October* 30, 1858.

[Many other instances have occurred within the last fortnight, but I have not room for them.—*E. Newman.*]

On the Food plant of Polyommatus Artaxerxes and P. Agestis.—Much has been stated by Mr. Logan and Mr. Wailes (Zool. 6276 and 6278) to prove that P. Agestis feeds on the Helianthemum; facts prove to me the contrary. That the perfect insects frequent the blossoms of that plant I do not dispute, but this does not prove that the larvæ feed on it. Now for a fact or two: the Deal sands (a tract of waste land about five miles in length by half a mile in width), from one end to the other, is a great station for Erodium cicutarium; here P. Agestis may be found in hundreds, and it is here that I take the larvæ. These sands are adjoining marshes of some miles in extent, on which there is no Helianthemum at all: leaving this part of the country, and travelling inland, we shall find the Helianthemum in abundance, and P. Agestis is quite wanting, or very rare. Another instance: at Dartford Heath, in Kent, the Erodium is found in some plenty; here P. Agestis may be found also; but the Helianthemum is rare on the Heath. Again, on the sides of Darenth Wood the Helianthemum grows in abundance; here but one or two of P. Agestis have been found, and they had evidently got astray. On the sand-hills P. Agestis and the common blue keep company; so fond are they of the Ammophila arundinacea that these two species are always found sitting on it to rest at night and flying among it by day, and one might arrive at the conclusion that it is their food-plant: seldom are they found on their food-plant, the Erodium. It is well known that the common blue settles on the common rush, but that does not prove it is their food-plant The reason why P. Artaxerxes is found in such numbers on Arthur's Seat is because it is a great station for Helianthemum vulgare. Mr. Logan says P. Agestis is as much attached to the Helianthemum as to the Erodium: I think these facts prove the contrary; or why is not P. Agestis found in plenty where the Helianthemum grows in abundance? Again, I am informed by Mr. Logan that the larva of P. Artaxerxes will spin up against the face of a rock or in confinement: I have bred a great number of P. Agestis, and have never found one so inclined. Does this not again prove the two species, by the difference of habitat of the larva? I should much like to see a larva of P. Agestis feeding on the Helianthemum: who has ever found one?—*H. J. Harding; 1, York Street, Church Street, Shoreditch.*

[I agree with Mr. Harding that the fact of an insect settling on the blossoms of a plant prove nothing as to the food of the larva: Sphinx Convolvuli obtained its name from the propensity of the imago to suck the honey of tubular flowers, but the conclusion was a most rash one, and has led hundreds to seek the larvæ among the leaves of the bindweed.—*E. Newman.*]

Double-broodedness of the Notodontidæ.—The observations of Mr. Newman and Mr. Boyd (Zool. 6281, 6283) on my paper on the "Double-broodedness of the Notodontidæ" (Zool. 6248) appear to have been written under an entire misapprehension. It is evident that both these gentlemen have the impression that the experiments with Pterostoma palpina and Notodonta dictæa commenced in May, 1858, and terminated with the larva in August of the same year. If I am correct in this supposition, they are in error on both points; they have overlooked the fact that the communication alluded to is a continuation of one published in the 'Zoologist' (Zool. 5826). These experiments commenced in August, 1857, with larva, and concluded in August, 1858, with the ova, embracing a period of twelve months, and carrying both insects through two entire cycles of life. I further mentioned, "almost parenthetically," that I had commenced a third cycle,—that is, that I had larvæ again feeding; this latter observation, however, was only made to show that the last state of the second cycle was complete,—namely, that the eggs had proved fertile,—but beyond this I attached no value to it whatever; these larvæ formed no part of my experiments, the two cycles were complete without them. Mr. Newman introduces Acherontia Atropos as an insect which he says I should consider double-brooded; on the contrary, this insect is entirely single-brooded, according to the rule laid down by Mr. Newman himself, and with which I fully agree: with Atropos no one of the four states occurs more than once in the twelve months; indeed, necessarily, if one state occurs more than once all must: it is true that individuals of this insect appear in autumn, the remainder in spring, but it is only an irregular, prolonged development from the same set,—namely the autumn-formed pupæ. Will Mr. Newman say from which of my papers he gathered that the divided or prolonged appearance of imago from the same set of pupæ was considered by me as evidence of double-broodedness? Mr. Newman further says that the "state of pupa (of A. Atropos) occurs twice." Now this may be said of every insect whose pupa continues in that state from autumn till spring, and although it may thus be found at two periods of a calendar year, it is only formed once in the four seasons, or twelve months. This insect has only one cycle of life in a natural year: the appearance of the imago in autumn is premature, and the individuals barren, dozens have been opened in this town during this autumn proving this fact. Mr. Newman again says "But even supposing the two cycles of existence were completed by a Notodonta in 1858" (meaning, I suppose, twelve months) "still the temperature of the past season having been abnormal the rapid progress of insect transformation may have been abnormal also." I hope this is the *dernier resort* of dying doubt; he appears to forget that my experiments with N. ziczac, which produced the same results as those with N. dictæa and P. palpina, were carried on during the years 1856 and 1857. Mr. Boyd says, "I hope Mr. Gascoyne will publish the results of his experiments when the cycle is complete." I have before shown that the double cycle was completed with the second production of ova. I will just recapitulate the experiments with P. palpina and N. dictæa, showing the two cycles, taking the dates roughly. Larva fed up September, 1857; pupæ remained through the winter; moths emerged in May, 1858; these laid eggs which hatched in June and proved fertile: this forms the first cycle. Larva began to feed in June and buried at the end of that month and early in July; moths appeared about the beginning of August; eggs from these hatched in about ten days: this completes the second cycle, and here ended the experiments. I may add that the larvæ from these duly fed up, and the pupæ of both P. palpina and N. dictæa are now taking their winter repose.—*G. Gascoyne; Newark, November* 4, 1858.

[Invited by Mr. Gascoyne to enter into further explanations, it is from no want of courtesy that I decline, but simply because I possess no additional facts to lay before my readers: my interpretation of Mr. Gascoyne's views may have been erroneous; at any rate he is their best interpreter. One word as to "exceptional" summers, the term applies equally to 1857 and 1858; in both the temperature was "exceptionally" high.—*Edward Newman.*]

Occurrence of Glæa erythrocephala on the South Coast.— My friend Mr. Wright has just brought to this office a specimen of this rarity, taken last night by himself at sugar on the south coast; two others were taken by a companion at the same time and place.—*Edward Newman; November* 2, 1858.

Occurrence of Camptogramma fluviata near Worthing.—I captured a splendid male of this insect, in the middle of October, near Worthing, Sussex. It seems to be a widely distributed species, and to occur throughout the summer and autumn months. —*H. Tompkins;* 44, *Guildford Street, Russell Square, London, November* 5, 1858.

A new Nepticula.—There is a species of Nepticula now in the larva state in apple-leaves, which I have repeatedly met with and reared. It does not seem to be as yet described; a brief account of it may therefore be interesting, and I would propose the name Pomella. The egg is deposited on the under side of apple-leaves; the larva, when hatched, mines in the upper cuticle, and usually in the direction of the stem, forming a slender track, which turns pink, and betrays the presence of a larva in the leaf before it is visible to the naked eye; this track is frequently close to a rib; as the mine becomes wider it is orange-coloured, with irregular tracks of brownish excrement: it is never very conspicuous from above, but quite invisible on the under side; after a time the larva commonly doubles on its track, and forms a blotch, from which it emerges on the upper side to wander away and form its cocoon. The larva is very shining, orange-coloured, with dorsal vessel only slightly darker; head pale chestnut. The cocoon varies from dark chestnut to bright orange: it is slightly mussel-shaped, and always has a rim of much brighter hue round the upper end, which gives it a striking appearance; the pupa is protruded on the escape of the perfect insect. The imago expands about three lines, and is of the unicolorous group; the anterior wings cinereous, glossy, and with a purple hue, brightest towards the apex; the posterior wings of an uniform paler colour; head rufous with whitish eye-caps. In the perfect state it is closely allied to N. pygmæella. It is double-brooded, appearing in May and August, the larva occurring in July, October and November; the July larva seems very scarce,—I observed them this July for the first time; the autumnal larvæ are by no means rare in the neighbourhood of Bristol.—*P. H. Vaughan; Redlands, Bristol, October* 29, 1858. —'*Intelligencer.*'

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